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THE FORTY-FOURTH KELVIN LECTURE

"THE DILEMMA OF LORD KELVIN"

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(*Lecture delivered before* THE INSTITUTION, 23*rd April*, 1953.)

The Kelvin Lecture was inaugurated in 1908, not only to commemorate the name of Lord Kelvin, but also to help members of The Institution to keep in touch with the latest developments in physical science. The first nine lectures were concerned with various aspects of the life and work of Lord Kelvin, but thereafter, with one exception, they have covered a variety of scientific subjects, and it is nearly 30 years since the last direct tribute to Lord Kelvin was paid in these lectures. Moreover, only once has the lecture been given by an occupant of the Kelvin Chair at Glasgow University.

Kelvin's occupation of that Chair was no mere accident of circumstances. Matriculating at Glasgow University at the age of ten, he was appointed Professor of Natural Philosophy when he was 22, and for no less than 53 years he continued in that office, pouring out a continuous stream of papers upon every aspect of physical science. The objectives and character of his work were profoundly influenced by the great industrial developments which surrounded him in the city of Glasgow. In contrary fashion, his influences at Glasgow still remain, 46 years after his death, and cannot fail to be ever-present in the mind of his successor.

For all of these reasons it seemed to be a natural choice that I should once more remind The Institution of the debt which all scientists—particularly, perhaps, elcctrical engineers—owe to this very famous and remarkable man. Owing to the great distinction of his scientific work, and still more to the multifarious nature of his activities and influences, a very considerable biographical literature about him is available, and Kelvin himself published no fewer than 661 scientific papers. It is clearly impossible to present anything approaching a comprehensive account of his work in a lecture of one hour's duration. But it seems to me that, by concentrating upon a very small number of these papers, by selecting a few of his records and sayings, and by placing this material in juxtaposition, certain points of view emerge which are of great interest in relation both to Kelvin's own life and work, and to the very general problems which confront scientists and organizers of science to-day.

Let us then picture William Thomson at the age of 22, fresh from his academic successes at Cambridge—which University he entered at the age of 17 after completing the courses at Glasgow—already the author of some 20 scientific papers and newly appointed to the Chair of Natural Philosophy at Glasgow. In his notebook appears the following entry:

Glasgow, Oct. 31, 1846. 11.45 *p.m.*

I have this evening (in the middle of my work finishing an introductory lecture) after thinking on Faraday's discovery of the effects of magnetism on transparent bodies and polarized light, been recurring to my idea (which occurred to me in the May term) which I had to give up, about magnetism and electricity being capable of representation by the straining of an elastic solid constituted in a peculiar way. I *think* the following must be true:

If particles along a closed curve of any form be displaced equal infinitesimally small distances along the curve, the displacement produced at any point of the medium can be represented in some way by means of the differential coefficients of the *solid* angle whose vertex is the point and base the closed curve. This solid angle is the potential due to the action of a voltaic current, circulating in the closed curve, as is known.

Then a bar magnet would be represented by an axis turned round in the elastic solid so as to drag points of the solid round along with it, etc. . . . I am not at all sure of anything I have written just now, but I want to get it out of my head, as I have no time to spare during the session. . . .

I have often noticed that the dominant impression which Kelvin left upon those who knew him personally was one of indefatigable energy. Tasks which the normal man would regard as wholly absorbing and necessitating serious allocation of working time were accepted and performed with an expedition which made it seem that to him they were trivial. It is interesting in this connection to note how he was occupied on the evening before his first lecture as Professor.

Two further entries in his diary refer to the same problem:

November 28. 10.15 *p.m.*

I have at last succeeded in working out the mechanico-cinematical (!) representation of electric, magnetic and galvanic forces.

November 29. 2.45 *a.m.*

I have just completed a paper for the Journal on this subject.

This paper, "On a Mechanical Representation of Electric, Magnetic and Galvanic Forces," was published in the *Cambridge and Dublin Mathematical Journal* in 1847. The paper starts from the idea that to every problem connected with the distribution of electricity upon conductors, or with the forces of electrical attraction and repulsion, there is a corresponding problem in the theory of elastic solids. For example, absolute displacement of a particle in the elastic problem corresponds to electrical attraction; angular displacement at a point, to magnetic force in a

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body; and a more complex displacement, to magnetic forces due to a current in a wire.

It is clear that Thomson is here suggesting only analogies between the elastic and electrical problems, but it is interesting to note that he was led to the examination of the problem by considering Faraday's latest experiments upon the effect of magnetic forces on polarized light in its passage through transparent bodies.

Shortly after this paper was published, Thomson met Faraday in London, and, following a conversation of which, I believe, no record remains, wrote a few days later to Faraday as follows:

June 11, 1847.

My Dear Sir,

I enclose the paper which I mentioned to you as giving an analogy for the electric and magnetic forces by means of the *strain*, propagated through an elastic solid. What I have written is merely a sketch of the mathematical analogy. I did not venture even to hint at the possibility of making it the foundation of a physical theory of the propagation of electric and magnetic forces, which, if established at all, would express as a necessary result the connection between electrical and magnetic forces, and would show how the purely *statical* phenomena of magnetism may originate either from electricity in motion, or from an inert mass such as a magnet. If such a theory could be discovered, it would also, when taken in connection with the undulatory theory of light, in all probability explain the effect of magnetism on polarized light.

Although Thomson disclaims in this letter any intention of presenting a physical theory of the propagation of electric and magnetic forces, I think there can be no doubt that already, as a young man of 23, he had conceived of an objective which he meant to make his life's supreme task. We must remember that Thomson had been bred in the age when Newtonian dynamics had for more than a hundred years constituted the sound bedrock of mechanical science, capable of accurately verifiable predictions over a vast range of terrestrial and celestial problems. The phenomena of interference of light had, during the early part of the 19th century, been shown to be explicable in terms of the mechanical indulations of a hypothetical medium. Heat had been shown to be a form of motion. Only the phenomena of electricity and magnetism had so far received no explanation which would bring them also under the aegis of mechanics. There can be little doubt that Thomson felt convinced that all of the branches of physical science were essentially based upon dynamics, and that he had set himself the task of evolving an all-embracing comprehensive theory which would afford a dynamical explanation of mechanical, optical, electrical and magnetic qualities, either by the arrangement and mutual actions of molecules, by structural concepts of molecules or by their relation to an all-pervading ether. It is also of great interest to note that this letter to Faraday was written 17 years before Maxwell published his famous paper which brought out the relationship between light and the electromagnetic field. The irreconcilable differences in viewpoint between Thomson and Maxwell will be discussed later, but here I think it fair to suggest that, even at this early date, Thomson's letter shows him to be within striking distance of Maxwell's solution.

Although it is likely that Thomson had at this time envisaged his life's main problem, a long period was to elapse before it again became his chief preoccupation. The industrial revolution was now in full swing, and the use of steam and power for industry and locomotion was assuming ever-increasing importance. Steam engines had been in practical use for over a hundred years, but the basic principles of their operation were quite obscure. Carnot, the brilliant young French engineer, had many years earlier written his famous paper upon the efficiency of steam engines and the reversible cycle, and in 1847 Joule read a paper at Oxford describing his experiments upon the "mechanical value of heat." At the present day, when the quantitative equivalence of heat and energy is so natural a concept, it is difficult to appreciate how contrary was such an idea to the mode of thought at that time. Carnot's theory had explained the efficiency of engines in terms of the fall of heat from one temperature to another without change in its total amount. This concept was clearly contrary to the requirements of Joule's experiments, where the mechanical work done by the engine would need to be related to the disappearance of an equivalent amount of heat.

In later years Thomson described his impressions thus:

> . . . But as I listened on and on I saw that (although Carnot had vitally important truth not to be abandoned) Joule had certainly a great truth and a most important measurement to bring forward.

Thomson talked to Joule after the meeting, and a charming picture of the domestic life of the 19th century scientist is provided by Thomson's further biographical note:

> . . . we became friends from thenceforward. However, he did not tell me that he was to be married in a week or so; but about a fortnight later, I was walking down from Chamonix to commence the tour of Mont Blanc and whom should I meet walking up but Joule, with a long thermometer in his hand and a carriage with a lady in it not far off.

For the next five years Thomson's main interests lay in the problems of thermodynamics, culminating in his famous enunciation of the second law of thermodynamics, which is still one of the most comprehensive laws of physical science and a most potent tool for discovery. It is true that Clausius was the first to appreciate that some heat may be transformed into work in the passage from source to refrigerator, without vitiating the most significant of Carnot's conclusions, but it was Thomson who gave the whole a comprehensive mathematical treatment of general applicability and who showed, by many examples in thermo-electricity, electricity and magnetism, how the law could be applied to explain and discover physical phenomena.

Although this work in the field of thermodynamics remains to-day as the most important contribution which Thomson made to the realm of pure science, and was his main interest from 1847 to 1852, he did not neglect electricity and magnetism, but published many papers giving a comprehensive and formal analytical development of the mathematical theory of magnetism in which the concept of magnetic inductive capacity (susceptibility) appeared for the first time.

The publication in 1853 of a paper "On Transient Electric Currents" marked the beginning of a new phase in Thomson's career. In this paper he considered the problem of the discharge of a condenser when earthed by a conducting wire. Noting that the potential energy, $\frac{1}{2}q^2/c$, must change during the discharge, he concludes that, according to the "preservation of mechanical energy," corresponding "mechanical effects" must be produced in the discharger, which he assumes to be the generation of heat and the "alteration of the electrical motion." To estimate the latter quantity he considers the "actual energy" of an electric current and estimates the magnitude of this by the quantity of work which would have to be spent against mutual electromagnetic forces in bending it from its actual shape to any other, while a constant current, γ, is maintained by an external e.m.f., together with the "actual energy" in the conductor so altered. He concludes that this "actual energy" must be proportional to γ^2 and sets it equal to $\frac{1}{2}A\gamma^2$, defining A as the "electrodynamic capacity." He then sets up the differential equation for the motion and finds that when $K^2/4A^2$ is less than $1/CA$ the discharge will be oscillatory. The expression

$$\frac{\pi}{\left(\frac{1}{CA} - \frac{K^2}{4A^2}\right)^{\frac{1}{2}}}$$

is deduced for the time interval between successive maxima of the current, and will be recognized as the completely correct expression for the time-constant of a *CLR* circuit, the basis of tuning in every radio receiver. Thomson goes on to consider how such oscillatory discharges might furnish the explanation of multiple flashes of lightning, and, still further, how such oscillations might be generated and made visible by rotating mirrors in the laboratory. We have here the approximate specification of the oscillators with which Hertz, 34 years later, first generated electromagnetic waves in the laboratory. I do not know whether at this time Thomson actually made any experiments of the type he envisages, but if the issues which I raised in connection with his letter to Faraday in 1847 are also considered, it is tempting to draw the conclusion that had he done so, and had he not been diverted from these topics by other interests, he might well have combined in his own life the work of Kelvin, Maxwell and Hertz, and so ranked in fame with Newton. However this may be, it is interesting to note that at the age of 29 Thomson had founded one science and made contributions to another which were to assume world-wide practical significance.

At the end of the paper to which I have just referred, Thomson considers the rise of current in a conductor to which an e.m.f. is suddenly applied. He was thus excellently prepared for the situation which arose in the following year (1854), when his attention became directed to the experiments of Latimer Clark on the retardation of signals in submarine telegraphy. Nearly 20 years had elapsed since the first telegraph had been put into commercial operation in England, and although a Dover–Calais telegraph had been instituted in 1851, grave difficulties due to signal attenuation and the consequent slowness of the rate of signalling were already being encountered and apparently set an insuperable obstacle to such signalling over much greater distances. Apart from the considerations in his "Transient Current" paper, Thomson had clearly recognized the similarity between Fourier's mathematical treatment of the flow of heat and the laws which would apply to the diffusion of a current along a wire. His mathematical ability, already so elegantly exhibited in connection with problems of the former type, was readily available to deal with this outstanding difficulty of telegraphic development. Showing that a sharp current pulse applied to one end of a cable caused only a slow rise and fall at the far end, and calculating the retardation in terms of the interval which must elapse between the first arrival of the current and its attainment of maximum value, he came to the conclusion that this retardation would be proportional to both the capacitance and the resistance of the cable. Thus the retardation would be proportional to the square of the cable length, and for a transatlantic cable would be about 10 seconds. He showed that by appropriate increases of insulation thickness and wire diameter, each in proportion to length, retardation could be kept the same for long and short cables. By 1856, when the Atlantic Telegraph Co. was formed, Thomson had already published several papers upon the subject. He was in that year elected to be one of the 18 directors of the company. Thus the die was cast which was henceforward to make of William Thomson the foremost exponent of the application of physics and to deflect his main interests and endeavour away from the task in pure science which he had envisaged in his youth and in which he had shown such outstanding ability and promise.

This conflict in the life of Kelvin between the claims of pure science and those of scientific applications was what I intended to convey in the title to my lecture. I must admit that I was considerably embarrassed when I had occasion recently to consult a dictionary, and discovered the formal definition of the word "dilemma." I found "a position where each of two alternative courses is eminently undesirable." Perhaps my face is somewhat saved by an alternative given, "a trying practical difficulty." There is no doubt that Thomson, himself, would have approved that definition of his cable project. But that was not what I meant to convey.

There is no doubt that Thomson found the cable project of absorbing interest. In a letter to Joule, written on the 25th September, 1858, when the cable, triumphantly completed only seven weeks earlier, had finally broken down after a steady deterioration, he said:

> Instead of telegraphic work, which, when it has to be done through 2 400 miles of submarine wire, and when its effects are instantaneous exchange of ideas between the old and new worlds, possesses a combination of physical and (in the original sense of the word) *metaphysical* interest, which I have never found in any other scientific pursuit—instead of this, to which I looked forward with so much pleasure, I have had, almost ever since I accepted a temporary charge of this station, only the dull and heartless business of investigating the pathology of faults in submerged conductors.

From the date of Thomson's association with the cable project, his interest became increasingly devoted to scientific applications, and the ultimate success of the endeavour added so greatly to his reputation and prestige that for the rest of his life ever-increasing claims of a technical and applicative character were made upon his time and energy. His advice was sought upon innumerable and widespread developments of electrical technology all over the world. We find him suggesting the location of electrical generating stations at pit-heads, advocating (regrettably) the transmission of power by direct rather than alternating current, consultant to the project of harnessing the Niagara Falls, adviser to foreign governments on telegraphic and power distribution, etc. But it must not be thought that he was never to return to his "comprehensive theory," or that his output of scientific papers on subjects ranging from problems of cosmogony to water waves and gyroscopes was in any way diminished. To his work in pure science I shall return later, and lest it be thought that I am wholly deprecating his deflection from complete absorption in pure science, I must first return to the cable development to pay tribute to its importance, and to the great influences which were exerted upon the development of electrical technology by Thomson's attention to the project.

On the value of the cable project itself there is no need for me to dwell. Although the cable, when it was finally laid after many abortive and heart-breaking attempts, functioned satisfactorily for only a few weeks, the possibility of transatlantic telegraphy was certainly established. In a speech in Glasgow, not long after this breakdown, Thomson said, "What has been done will be done again. The loss of a position gained is an event unknown in the history of man's struggle with the forces of inanimate Nature." Nearly eight years were to elapse before this position was regained, but thereafter the cable functioned well and provided a most valuable link between the Old and New Worlds. But what I might term the subsidiary consequences of the cable project must also be emphasized.

When Thomson first became a director of the Atlantic Telegraph Co. the contracts for the cable had been placed and manufacture was already in progress. Thomson's first encounters with work in hand showed him that the specifications of the cable were wholly inadequate. No reference to the resistance of the copper conductor appeared in these specifications, the company's chief electrician, Wildman Whitehouse, being convinced that the resistance was unimportant. The gutta-percha insulation was of insufficient thickness to satisfy Thomson's minimum-capacitance requirement; no proper processing of the application of the insulator to the conductor had been developed, and, indeed, in many completed lengths of the cable the conductor was far from central within the insulation.

Thomson's attack upon this position was characteristic of the

man. He instituted investigations at Glasgow upon copper samples from the cable, and finding that these varied considerably in conductivity, proceeded systematically to investigate possible causes, such as the influence of twisting and straining of the conductors. Since the copper was known to be of about 99% purity, any influence of impurity upon conductivity was unexpected, but with characteristic thoroughness Thomson submitted samples to Hofmann for analysis. Measurements of the conductivity of the same samples gave the following results, showing clearly that the purity of the copper constituted the essential criterion in this problem:

Conductivity (arbitrary units)..	42	71·3	84·7	86·4	102
Impurity content, %	1·24	0·80	0·47	0·43	0·10

To convince the company of the need for a specification of conductivity in the cable contracts called for much perseverance, and we are told that Thomson deliberately obstructed all other business at board meetings until it was agreed to insert such clauses. We are also told that a suggestion was offered by the Prince Consort that the cable should be insulated in flexible glass, and that, when Thomson suggested that no such useful insulator existed, the Prince Consort quoted the authority of Petronius Arbiter, who once held the office of Director in Chief of Imperial Pleasures under Nero. After the insertion of conductivity clauses into the cable specifications the contractors declared that supply under such conditions was impracticable, whereupon Thomson himself supervised the setting up of electrical testing apparatus at the factories. After this the contractors agreed to continue supply, although at increased cost.

I have dwelt upon this aspect of the cable project because I imagine it to be the first introduction of proper scientific specification into electrical engineering practice, and also perhaps the first introduction of physical testing laboratories into factories engaged in electrical manufacture. Thomson's interest in precise measurements and standards of electrical quantities was maintained in later life, and towards the end of the 19th century he took an important part in the formation of a Government electrical standardizing laboratory. In 1902 he was present and spoke at the opening of the National Physical Laboratory.

Another most important consequence of Thomson's interest in the cable project lay in the impetus he gave to the design and manufacture of sensitive electrical measuring instruments. The early instruments used for the detection of the telegraph signals were very insensitive, and, in view of the attenuating effect of the cable, necessitated very slow rates of signalling. With the aid of his university students Thomson developed greatly improved designs of galvanometer and applied a rotating mirror to magnify the magnet deflection. The third consequence of importance in this respect is the use he made of university students in these researches. This practice developed into the formal teaching of experimental physics to students at Glasgow, and I believe that this University was the first to institute such training, now rightly regarded as an indispensable feature of a scientific education.

I know it is customary, Mr. President, for Institutions to be venerated for their antiquity, and I am often perplexed by this, because it seems to me that great age should rightly demand proportionate accomplishment. The Institution of Electrical Engineers is perhaps young enough to appreciate this point. To my mind the most remarkable feature of electrical developments is the outstanding progress which has been made in so short a time. In viewing the achievements which had been made in electrical technology during Kelvin's lifetime, I find it almost unbelievable that Ohm's law was not discovered until after William Thomson was born. The pride which every electrical engineer must feel in the fantastic rate at which electrical discoveries have been put to the service of man is indeed justifiable. The outstanding role which Kelvin played in this record of successful electrical achievement is surely worthy of the tribute which, on your behalf, I pay to him to-day in this lecture.

I must now return to the consideration of Kelvin's work in pure science, and in particular to his quest for a "comprehensive theory." In his Presidential Address to The Institution in 1889, entitled "Ether, Electricity and Ponderable Matter" and delivered more than 20 years after the publication of Maxwell's electromagnetic theory, Kelvin's approach to these problems was still basically in terms of the search for mechanical analogies of electrical and magnetic effects. He repeats verbatim the closing paragraphs of his 1847 paper, which said that he could not then

> . . . enter into a special examination of the states of a solid body representing various problems in electricity, magnetism and galvanism, which must therefore be reserved for a future paper.

He then adds:

> As to this last sentence, I can say now, what I said forty-two years ago—"*must be reserved for a future paper!*" I may add that I have been considering the subject for forty-two years—night and day for forty-two years. . . . I have been trying many days and many nights, to find an explanation, but have not found it.

Kelvin then proceeded to demonstrate with gyroscopic models and webs of cloth and wool to illustrate possible structural concepts for the ether which might be of service in providing that medium with the properties requisite for his purpose. Early the following year the promised paper appears at last, not however in the normal manner, but in Volume III of his "Mathematical and Physical Papers," together with reprints of many other papers, some of which bore very early dates. In this paper the ether is endowed with a *quasi* rigidity depending upon an inherent *quasi* resistance to rotation. The subject is developed mathematically and concludes:

> Hitherto our representation has been purely static. An obvious kinetic extension is expressed by . . . We thus have simply the *undulatory theory of light*, as an inevitable consequence of believing that the displacement of elastic solid by which, in my old paper, I gave merely a "*representation*" of the electric currents, and the corresponding magnetic forces, is a reality.

The subsequent sentences show, however, that he is still far from his original objective. Electricity had to remain a separate entity or concept, needed, in his theory, to transmit force from matter to ether. He concludes:

> I see no way of suggesting properties of matter, of electricity or of ether, by which all this, or any more than a very slight approach to it can be done and I think we must feel at present that the triple alliance, ether, electricity and ponderable matter is rather a result of our want of knowledge, and of capacity to imagine beyond the limited present horizon of physical science, than a reality of nature.

Although Kelvin fully accepted and admired the Maxwellian developments of the electromagnetic equations, it is clear that to his mind these in no way constituted a "theory" and that Maxwell's approach was from the wrong end. To him, the ether was an unquestionable reality, amply established by the undulatory phenomena of light and undoubtedly the medium which, endowed with the necessary properties, would exhibit the phenomena of electricity and magnetism, and even perhaps of ponderable matter. Maxwell's approach had more limited boundaries; basic concepts of electric and magnetic fields and of displacement currents (so unsatisfactory to Kelvin, the engineer) were developed to explain the phenomena of light. The attitudes of mind were irreconcilable. As Kelvin said in his famous series of lectures at Baltimore:

> I never satisfy myself until I can make a mechanical model of a thing. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory. I firmly believe in an electromagnetic theory of light, and that when we understand electricity and magnetism and light we shall see them all together as parts of a whole. But I want to understand light as well as I can without introducing things that we understand even less of. That is why I take plain dynamics.

And again in a lecture to the Royal Society of Edinburgh in 1900, speaking of the electromagnetic theory:

> . . . it demands of us not merely an explanation of all the phenomena of light and radiant heat by transverse vibrations of an elastic solid called ether, but also the inclusion of electric currents, of the permanent magnetism of steel and lodestone, of magnetic force and of electric force in a comprehensive ethereal dynamics.

With the development of relativity, the requirement for an ether in physics disappeared, but it is interesting to note that as recently as 1951 Dirac has argued that the existence of an ether is not incompatible with relativity theory. Furthermore, Dirac's new theory of electrodynamics requires that an electric charge must flow with a certain determinable velocity and that for regions where no charge exists this velocity is that with which a charge would flow if it were introduced. A certain velocity at all points in space-time therefore plays a fundamental role in electrodynamics. It is natural to assume that this velocity is the velocity of something, and so once again a requirement perhaps emerges for an ether in physical theory.

At the official banquet of the Jubilee held at Glasgow in 1896 in Lord Kelvin's honour, the gathered assembly were surprised to hear this celebrated man, author of more than 660 scientific papers, recipient of 22 honorary degrees and of honours of many other kinds from countries all over the world, say in his address:

> One word characterizes the most strenuous efforts for the advancement of science that I have made perseveringly during 55 years. That word is failure.

What he meant will be clear to you from what I have said in this lecture. Ridiculous though it may seem to apply such a word to the career of such a man, it must be admitted that in one sense it conveys a truth: Kelvin had failed to attain his objective. No doubt the reason lay partly in the unlimited character of the problem which he hoped to solve. Progress in science is normally achieved by drastic limitation of the field of study. Yet Kelvin's lack of progress in pure science in his later years must also be attributed in some measure to the absorption of his time and energy in the applications of science and perhaps, even more, to the practical attitude of mind which was fostered and developed by the successes which attended his work in the fields of such applications. That he was capable of a different approach was shown as a young man in his famous work upon thermodynamics. But Kelvin acquired an ever-increasing belief in the finality of engineering concepts.

Despite the successes which attended Rutherford's rather similar approach to the problem of atomic structure, it must be recognized that the great developments of physical science which have taken place during the past 50 years have, in the main, been based upon much more recondite considerations. In this connection I need refer only to the geometrization of mechanics, the development of relativity, wave-particle duality and the developments of quantum theory. I hope that it will be no affront to electrical engineers if I suggest that the methods which they apply with such success, and even the mental approach to the problems they solve, are not necessarily appropriate to the progress of pure science. The fruits which the engineers bring to perfection and make available to mankind are not always predictable when the tree is planted.

At the present time much official attention is being paid to the encouragement of applied science, to the need for technological institutes and to the sponsoring of applications of science. The need for such an attitude is unquestionable, but it is disturbing to read that the Department of Scientific and Industrial Research is expressing the view that the claims of pure science in their programme are suffering from neglect on this account. That pure and applied science must march hand in hand is an obvious truth, but I wish to suggest that each must, of necessity, be tended as carefully as the other, and that the organization of the interplay between these two great endeavours calls for the most careful and intelligent planning. Some of the great contributions which Kelvin made to electrical applications have been emphasized in this lecture. I hope that nothing that I have said about his work in pure science will be regarded as showing any lack of appreciation on my part of the importance and permanence of his major contributions in that field. But great though his reputation as a pure scientist may justifiably be, I think that the best short assessment of the overall character of his work was that made by the President of the American Society of Civil Engineers at the Kelvin Jubilee:

> We recognize in Kelvin the great leader who brought together pure science and applied science, who brought the profoundest conceptions of science into the service of the constructive arts.